Dr. Marshall Hall's yacht *Norna* off the west coast of Europe, no animal forms have been discovered, so far as I am aware, identical with chalk fossils. Additional evidence has, however, been procured that over a large part of the bottom of the Atlantic a deposit is being formed mainly of disintegrated globigerinæ and other foraminifera and coccoliths, which appears to be undistinguishable from the ancient chalk.

Not fewer than twenty genera of vitreous sponges have been dredged belonging to two groups, the Hexactinellidæ and the Lithistidæ of Oscar Schmidt, both of which groups are highly characteristic of the chalk and greensand. These sponges are in vast numbers, like the ventriculites and their allies in older Cretaceous beds; and they, with other silicious organisms equally abundant in the modern chalk area, seem to be capable of supplying that amount of silica in a fine state of division which might explain the production of chalk flints. A large series of echinoderms were found, recalling to a remarkable degree, from the profusion of Cidarids and of star-fishes of such genera as Archaster, Astrogonium, and Stellaster, the general facies of the chalk echinoderm fauna; and besides this general resemblence, members of several families have been recovered which were supposed to be extinct. *Salenocidaris varispina* A. Ag., dredged by De Pourtales in the Strait of Florida, is a living Salenia; *Echinolampas caratomoides* A. Ag. perpetuates some of the most marked characters of the Galeritidæ. *Pourtalesia,* a genus first found by Count Pourtales and afterwards in the *Porcupine* expedition, is a true Dysaster. *Porocidaris purpuratus,* a fine species dredged off the Butt of the Lews, represents a genus hitherto known only by some isolated plates and radioles. Two very remarkable generic forms, dredged from the *Porcupine* off the coasts of Scotland and Portugal, only known from some fragments in the English white chalk, found a new family near the Diademidæ. Some new ophiurids approach their fossil ancestors; and off the coast of Portugal the dredge brought up at one rich haul twenty or thirty examples of a fine *Pentacrinus*; while over the whole area *Rhizocrinus loffotensis,* a degenerate little Bourgueticrinus, seemingly one of the last of the pear-encrinites, is abundant.

I am not in a position to say much about those groups which I have not personally examined, except that Mr. Gwyn Jeffreys and Prof. Martin Duncan report that among the mollusca and corals many species occur which have been hitherto known only as fossils, principally, as might have been expected, in comparatively shallow water forms in the Tertiaries.

I do not see that there is any object in attempting to explain this singular resemblance between these deep-sea deposits in the Atlantic and the old chalk in composition and structure and in embedded fauna on any other assumption than that of a continuity of conditions over some part at all events of the area. During the lapse of time, while the fauna of shallower water has again and again undergone almost total change by changes in the distribution of temperature and in the distribution of sea and land, the fauna of the deep water has been also affected. To a depth of 5,000 feet it is at present heated over a large portion of the North Atlantic many degrees above its normal temperature. Accepting, as I believe we are now bound to do, some form of the doctrine of the gradual alteration of species through natural causes, one is quite prepared to expect a total absence of the identical forms found in the old chalk. The utmost which might be anticipated is such a resemblance between the two faunæ as might justify the opinion that the later fauna bears to the earlier the relation of descent with extreme modification. Sir Charles Lyell asks if we have dredged belemnites, ammonites, baculites, hamites, turrilites, &c.; that question is, I think, best answered by the record of the old Cretaceous beds themselves, which are scarcely more remarkable for the presence of these singular and beautiful forms than for their rapid extinction. According to the view which I have felt myself compelled to adopt, the various groups of fossils characterising the Tertiary beds of Europe and North America represent the constantly altering fauna of the shallower portions of an ocean whose depths are still occupied by a deposit which has been accumulating continuously from the period of the pre-Tertiary chalk, and which perpetuates with much modification the pre-Tertiary chalk fauna. I do not see how this view militates in the least against the "reasoning and classification" of that geology which we have learned from Sir Charles Lyell. Our dredgings only show that these abysses of the ocean which Sir Charles Lyell admits in the passage quoted above to have outlasted on account of their depth a succession of geological epochs, are inhabited by a special deep-sea fauna possibly as persistent in its general features as are the abysses themselves.

WYVILLE THOMSON

Ocean Currents

ATTENTION has been much drawn of late to the subject of Ocean Currents and their causes, and it has occurred to me that there is a directing if not an originating cause of these streams, which has, so far as I am aware, been overlooked by physicists. It is known* that at some parts of the earth's surface there exists an atmospheric pressure capable of sustaining a column of mercury in the barometer of upwards of 30 inches in height; at the same time there are certain areas over which this pressure is only such as to raise the barometric column to a little over 29 inches. Now if we compare the difference of absolute weight sustained by two such areas, we shall see that in the space over which the higher atmospheric pressure exists, there is an excess of weight of air, amounting in round numbers to 1,000,000 of tons on each square mile. Applying this fact to the region of the ocean in which the surface currents are best known, the North Atlantic, we find from the isobaric chart that there is throughout the year over a large portion of the eastern side of this sea, next the coast of North Africa, a pressure (to use the convenient mode of expressing it) of upwards of 30·2 inches. To westward of this space, towards the Gulf of Mexico and the coast of the United States, the average pressure decreases; between Newfoundland and the British Isles the pressure is still diminished, till in the wide channel between Iceland, Norway, and Spitzbergen, we arrive at a yearly pressure of less than 29·6 inches. It is reasonable to believe that the waters which lie under the high pressure area have a tendency to escape from under the excessive weight, towards the space over which the pressure is less. But the high pressure area next the African coast is precisely that upon which the north-east trade winds descend, and the waters, aided in their choice of an exit, will naturally flow off to south-westward before the wind. Their continuance in this direction is barred, however; for across the whole of the southward passage between Africa and South America, there exists another belt of high pressure, out of which the south-easterly trade winds blow. The only course left for the escaping waters (allowing for the moment that the excess of pressure *is* a cause of their movement) is to westward, where the pressure is lessened, towards the Gulf of Mexico, and the east coast of America, and thence towards the low pressure space between Iceland and Norway. But this is exactly the course that the Gulf Stream, or rather the North Atlantic warm stream of which the Gulf Stream is the most prominent feature, is seen to take. Are we not then warranted in concluding that the difference of atmospheric pressure has some power both in originating and in directing the course of this ocean current?

In suggesting the unequal distribution of atmospheric pressure as a supplementary cause to difference of temperature and of density, to evaporation, rain, and winds, and to whatever further agents there may be in the production of ocean currents, I would venture to express a hope that some one in authority, by carefully comparing and valuing the power of each one of these motive forces, and their application to the known streams, will give to the world a system of the causes of ocean currents which will be vastly more relevant to the phenomena these streams are known to present, than any one of the theories which have as yet been put forth.

When we know that Sir John Herschel gives to the winds the entire right of setting the ocean streams in motion; that Captain Maury holds the universal circulation of the sea to be caused by nothing else than the differences in its specific gravity, and that Dr. Carpenter (or rather Professor Buff) would bring about a general interchange of polar and equatorial water by the aid of sunshine and frost alone; is it not time to ask which of these three causes we should accept as the true one, or if all three are partially concerned, what part is to be taken from each to let the others have their fair share in the work?

KEITH JOHNSTON, Jun.

The Measurement of Mass

I AM happy to meet with an opponent who comes so directly to the point as my Reviewer, W. M. W.

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* I would refer those who desire to look more particularly into this matter to the monthly isobaric charts prepared by Mr. Buchan, to illustrate his admirable paper on the mean pressure of the atmosphere. Trans. Royal Soc. Edin., vol. xxv.

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The gist of his argument lies in his assertion, that "if a true pound, as determined at London, were carried to the North Pole, it would weigh more than a pound."

Now, since the determination of a pound is actually effected by making it a copy of a standard so that they shall counterpoise each other *in vacuo*, it is strictly independent of locality.

The standards actually employed for this purpose, for example, the authorised copies of the principal standard pound which have been sent to various countries, have been made as nearly identical in mass as skilled workmen could make them, and have been sent at random to different latitudes.

Similar remarks apply to the weights in a chemist's box. Surely it is not seriously proposed that the chemist should file them down to accommodate them to the increase of gravity, when he takes them from London to Edinburgh.

Where should we find ourselves, if the makers of chemists' weights endeavoured to make them of different masses, according to the places where they were to be used?

Fortunately, this has never been attempted; and since all parts of the world are in possession of practically identical standards of mass, under the name of standard pound or standard kilogramme, and tolerably accurate copies of these and their fractional parts are in everybody's hands, why not acknowledge them as standards of mass, which they are in point of fact, although, according to the theory which I am combating, they ought not to be?

Those who hold that theory must choose between two evils:—they must either make the pound a unit of force, in which case they must file or load their weights as they go from place to place (this seems to be the alternative which W. M. W. chooses); or they must accept the gravitating forces of equal pieces of metal as nominal units of force at the different localities where these pieces of metal may happen to be, although these forces are really not equal. This latter alternative, which gives a variable unit of force, has been commonly adopted till recently, and a variable unit of mass has been conjured up to suit it.

If our spring-balances were as accurate as our standard weights, W. M. W.'s idea would be practicable. The equality of two forces at different places could then be very directly tested; but, in fact, the most accurate means we possess of making such a comparison consists in a double process, a weighing with the ordinary balance, combined with a difficult and less accurate pendulum-comparison. Inasmuch then as ease and accuracy of comparison is the first essential of a scientific standard, I submit that the world is right in employing standard pounds of equal mass and not standard pounds of equal gravitating force.

Much confusion arises from using the word *weight* in a connection which leaves it doubtful whether *mass* or *gravitating force* is meant. I trust you will keep your columns open for the further discussion of this question, as it much needs ventilation.

Belfast, January 11 J. D. EVERETT

The Tails of Comets, the Solar Corona, and the Aurora considered as Electric Phenomena

In reference to a letter from Mr. Bedford, Ph.D., published in your last number, allow me to state—

1. That I had never seen or heard of Dr. Bedford's theories.
2. That, judging from the extract given in his letter, Dr. Bedford has not published anything analogous to the electrical hypothesis which I have put forward. In his letter he has misquoted and made omissions to the extent of one-half the heading, in order, I suppose, to avoid the very mention of the word electricity, which, on the other hand, is the very substance of my postulate. OSBORNE REYNOLDS

Owens College, Jan. 17

Apparent Size of the Moon

From almost any place in the balcony of St. James's Hall, at an evening performance, one may find in the coronets of gas-jets, forming one or more of the arches across the roof, corroboration of what I wrote in NATURE of May 12, 1870 (vol. ii. p. 27). The nearest or first coronet, and the two next, observe a gradation of increase in apparent magnitude, as they should do. But the rest, which (unlike those three) descend towards the horizon, and should, nevertheless, observe a gradation of increase, are apparently all of a size. I call attention to this "unconsidered trifle" as having the full force of a very elaborate experiment. C. M. INGLEBY

Ilford, Jan. 7.

Atmospheric Currents

The following is part of a letter signed "G." in NATURE of 6th October, 1870:—

"It is very important to obtain correct and copious data regarding the atmospheric currents between say 5,000 feet and five miles above the level of the sea, and especially at various points on and near the equator, and at about 30° to 32° North and South latitudes. Within these limits the rain-bearing currents of the atmosphere move. If self-registering meteorological instruments were placed permanently upon several of the leading mountain ranges of the world, and their records copied at stated intervals, we should obtain valuable data for determining the direction, velocity, and magnitude of the controlling atmospheric currents of the globe."

This suggestion is most valuable, but it would involve a source of error that would be difficult to allow for. Most mountain ranges occupy so extensive an area that they have their own local climates, and indications on such ranges would consequently not give accurate information about the currents where they are not modified by such influences. The most valuable information will be obtained from the most isolated mountains. The most isolated mountains of sufficient height, within sufficiently easy reach of us, are Etna and Teneriffe. Etna is in the variables, and Teneriffe in the trade-winds.

I do not, however, agree with the following suggestion, that more valuable observations still might be obtained by means of captive balloons. Balloons cannot be used in stormy weather, and they are also subject to the great disadvantage, that they tell nothing about barometric fluctuations: because the height of a balloon above the earth can be known only from the barometer, and consequently there is no way of ascertaining the indications of the barometer at a known height. It is not at all certain that the barometric curves at the summit and at the base of Etna or Teneriffe would present any close correspondence. Sets of barometrical and other meteorological observations taken for sufficiently long periods at the summit and at the base of such mountains would be probably the most valuable of all data for meteorological science in its present state.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Jan. 14

THE AMERICAN ECLIPSE EXPEDITION

That portion of the American Eclipse Expedition which was stationed at Xeres was favoured by weather which enabled the observers to examine the Corona during the whole period of totality, and with some results of interest; an informal account of a part of which the kindness of Professor Winlock enables me to lay before your readers.

The party was in charge of Professor J. Winlock, and stationed in an olive grove about a mile from the city, the property of Mr. Davies, of Xeres, to whom all the members of the expedition are indebted for continual aid and kindness. The observations for the determination of longitude, latitude, and times of contact were conducted by Assistant G. W. Dean, of the Coast Survey, aided by Captain Ernst, of the U.S. Engineers, and by Mr. Gannett, of the Harvard College Observatory.

Their labours were facilitated by the courtesy of the Director of the neighbouring Observatory of San Fernando, with which telegraph connection was established, and the results attained will be found in full in their official report.

Besides the instruments of precision, the party was provided with four equatorial telescopes of from six to eight inches aperture, driven by clockwork, and many smaller ones. One of the largest telescopes carried an objective specially corrected for actinic rays, with which, and a companion instrument, photographs were taken during the totality and previous stages by Mr. Willard and his assistant.

In addition, a photographic telescope of upwards of thirty feet focus, placed horizontally, and receiving the solar rays from a heliostat, was used, in charge of Mr. Gannett.

The accessories of the instruments were such as the previous experience of the observers (nearly all of whom had witnessed the eclipse of August 1869) suggested, and were too numerous for description here, though a means

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